intrinsic factor may play the major part in effecting B_{12} absorption. Assuming that the active carbohydrates and the intrinsic factor are activating similar absorption mechanisms in the gut, these carbohydrates should make excellent tools for investigating the B_{12} absorption mechanism.

With the facts that are now available, it is felt that at least two theories of the mechanism of action of D-sorbitol and of the other active carbohydrates in promoting vitamin B_{12} absorption are possible: (1) the formation of B_{12}-carbohydrate complex with the resulting protection of the vitamin from more rigid binding with other substances, or (2) the stimulation of an absorption mechanism in the gastrointestinal tract.

[1] Castle, W. B., and Townsend, W. C., *Amer. J. Med. Sci.*, **178**, 764 (1929).
[2] Pitney, W. R., and Beard, M. F., *J. Clin. Nutr.*, **2**, 89 (1954).
[3] Chow, B. F., Horonick, A., and Okuda, K., *Amer. J. Clin. Nutr.*, **4**, 434 (1956).
[4] Boger, W. P., Wright, L. D., Strickland, S. C., Gylfe, J. S., and Ciminera, J. L., *Proc. Soc. Exp. Biol. Med.*, **89**, 375 (1955).
[5] Schilling, R. F., *J. Lab. Clin. Med.*, **42**, 860 (1953).
[6] Rosenblum, C., Chow, B. F., Condon, G. P., and Yamamoto, R. S., *J. Biol. Chem.*, **198**, 915 (1952).
[7] Latner, A. L., *Biochem. Soc. Symposia*, **13**, 69 (1955).

AN EXPERIMENT ON 'TELEPATHY' USING TELEVISION

By DONALD MICHIE
Royal Veterinary College, University of London
AND
D. J. WEST
Society for Psychical Research, London

THREE variants of perception without the aid of the known senses are commonly distinguished: 'clairvoyance' (extra-sensory perception of unwitnessed events), 'telepathy' (extra-sensory perception of events witnessed by a human agent) and 'precognition' (extra-sensory perception of future events, either in a context of clairvoyance or of telepathy). Although claims concerning these were considered by the majority of the contributors to a recent symposium[1] to deserve serious attention, they are still viewed with suspicion, the three main reasons being: (1) a phenomenon is claimed for which no one has yet suggested a convincing material basis; (2) such claims have traditionally been the preserve of superstition and crankery; (3) the phenomena elicited under experimental conditions are usually of small absolute magnitude, however long the statistical odds may be made by the sheer weight of the number of trials conducted in a given experiment; moreover, the conditions under which they can be reliably and consistently repeated have yet to be found.

In our opinion the first two reasons may be dismissed out of hand, for they could equally have been urged against many newly discovered phenomena which were later shown to be both real and important, for example, hypnosis. The third reason is a weighty and serious one. We ourselves are inclined to put it even more strongly: so long as extra-sensory perception remains a 'marginal' phenomenon it will matter little whether it is real or not. For even if it is real, experimentation along traditional lines is unlikely to lead to a causal analysis, and hence to control, of the phenomenon.

Faced with a similar impasse, investigators in other fields have found success along two main lines of attack, namely, selection of material on the basis of preliminary tests, and transformation of unselected material by special treatments. The stage hypnotist bases his success on his ability to pick from a large audience a few hyper-suggestible individuals; the psychiatrist can render almost any individual hyper-suggestible with the aid of the appropriate drugs.

The development of broadcasting, in particular of television, offers an opportunity of applying the method of selection or 'screening' on a mass scale. We have made a small-scale trial with the object of testing any generalized extra-sensory perception effect and looking for individuals with strongly manifested telepathic abilities who could then be made the subject of further experimental investigation. The results, of which a detailed account has been published elsewhere[2], are summarized below.

The 'screening' experiment was carried out on the evening of April 19, 1956, during a programme produced by Science Television, Ltd. After a brief explanatory discussion, one of us (D. J. W.) introduced to the audience the 'agent', Mr. G. W. Fisk, who was to retire from view and look at a series of 20 picture cards, keeping time with a signal given at 5-sec. intervals. The viewers were informed that the cards would be drawn at random from a pack consisting of three types, depicting a canoe, a wheelbarrow and a trumpet respectively. They were asked to record their guesses on a form printed in the *TV Times* and post it to the programme contractors.

1,367 completed records were received, transferred to punch cards and mechanically sorted. The pooled results showed no significant deviation from chance expectation. But one entry, submitted by a Mr. B. Downey, with 15 guesses correct out of 19 (the first card was a 'dummy') was considered sufficiently suggestive to justify further tests.

Apart from two runs each of 25 guesses with a 'Zener' pack, the results of which were negative, the further tests were conducted with a pack of 12 clock cards[2]. Each card showed a clock face with a single hand pointing to one of the 12 hours. The expected average divergence of guess from target is thus 3, with a minimum of zero and a maximum of 6.

These tests occupied a period of several months and comprised 239 guesses. In some tests the agent was in the same room as Mr. Downey in his home in Burton-on-Trent. In others the agent was in London and the percipient was in Burton-on-Trent. In 13 tests G. W. Fisk, and in 15 tests D. J. W., acted as agent. The identity of the agent in the 'distance tests' was always known to the percipient. The tests were eventually terminated because of the increasing apathy of Mr. Downey, who felt that no results of interest were accruing.

Analysis of the results by one of us (D. M.) showed no significant difference with either agent between

'distance' and 'same-room' tests. But there was a highly significant difference ($P < 10^{-3}$) between agents. The 132 guesses made with D. J. W. had an average divergence of 3·09 ($P > 0·5$). But the 107 made with Fisk (who had been the agent in the original 'screening' experiment) averaged 2·21 ($P < 10^{-5}$). This rate of scoring is comparable in magnitude with the effects reported by other workers in tests with specially gifted subjects[4]. It was maintained when the distance tests were viewed in isolation, and these were sufficiently numerous to be significant in their own right at the 1 per cent probability-level. This is in agreement with earlier work, and suggests that the emission of information by the agent is not like the emission of unbeamed radio or light signals, which fall off in intensity in proportion to the square of the distance.

The effect was in part due to an increased frequency of 'direct hits' (zero divergence), and in part to 'near misses' (that is, greater frequency of small than of large divergences between target and guess). This result adds another item to the list of phenomena which telepathy apparently does not resemble: it is not like the performance of a man trying to read a series of symbols in a completely dark room intermittently illuminated by a good light, unless he deliberately adulterates his direct hits with near misses. It is more like that of a man trying to read in a half-light on the margin of visibility.

Provided that the good faith of Mr. Fisk and ourselves, all of whom at some stage had access to the records, is accepted, the results suggest that television broadcasts can be used for picking out exceptional individuals whose extra-sensory perception is sufficiently strong to make causal analysis feasible. It also seems that potential agents as well as percipients should be 'screened'.

[1] Ciba Foundation Symposium on Extrasensory Perception (London, 1956).
[2] Michie, D., and West, D. J., *J. Soc. Psych. Res.*, **39**, 113 (1957).
[3] Fisk, G. W., and Mitchell, A. M. J., *J. Soc. Psych. Res.*, **37**, 1 (1953).
[4] Soal, S. G., and Bateman, F., "Modern Experiments in Telepathy" (Faber, 1956).

NOISE AND THE VISUAL THRESHOLD

THERE have been two interesting recent attempts to treat the visual threshold as a problem of discriminating a signal from a noisy background[1–4]. It is known from neurophysiological studies[5] that there is a variable resting discharge in the completely unstimulated eye. This 'retinal noise', together with any noise that may be added more centrally, forms the 'dark noise' which, on this theory, limits the sensitivity of the eye at absolute threshold. If there were no dark noise, there would be no reason why a single absorbed quantum of light should not be seen; if, however, the threshold of vision were set as low as this in the presence of dark noise, there would be a 'false-alarm rate', since the threshold criterion would be exceeded by the dark noise in the absence of external stimulation: thus the greater the variability of the resting discharge, the higher must the threshold be set to keep the false-alarm rate low. It is therefore important to know what is the average value and, more especially, the variability of the resting discharge.

The mean and variance of the resting discharge of single fibres can be obtained from direct electrophysiological studies. It would, however, be difficult to relate these figures to psychophysical thresholds and it would be useful to obtain estimates of the properties of the resting discharge from the psychophysical experiments themselves. Barlow[1,2] has tried to estimate simultaneously the variance of the resting discharge and the number of quanta necessary for vision from the shape of the frequency of seeing curve. His method is very ingenious but is also, unfortunately, rather insensitive and he was unable to estimate either of these quantities with any precision.

It is therefore very interesting that Gregory[3,4] has attempted to tackle this problem in another way. He has found that in differential threshold experiments, Weber's law:

$$\Delta I/I = C \qquad (1)$$

where C is a constant, does not hold exactly, but that when I is not too small, a much better fit is obtained by the relationship:

$$\Delta I/(I+k) = C \qquad (2)$$

where k is about 0·03 ft. lambert. In order to explain this, he supposes that r, the mean neural impulse-rate (using this term in a loose sense) corresponding to incident light of intensity I, is proportional to $\log(I+k)$. He also supposes that "the brain demands a constant fixed difference between impulse-rates arising from the comparison fields to make an intensity discrimination to a given fixed criterion". The difference between the mean neural impulse-rates is proportional to $\log(I+\Delta I+k)-\log(I+k)$, which when I is not too small is approximately $\Delta I/(I+k)$ (this can be shown by expansion in a Taylor series). Setting this equal to a constant, the observed relationship (2) is obtained.

This explanation of the empirical relationship (2) is the same as Fechner's explanation of Weber's law (1), except that r is supposed to be proportional to $\log(I+k)$ rather than $\log I$. It seems to us, however, that Gregory attaches a significance to the actual value of the constant k which it does not possess. For he says that "it is tempting to regard k as arising from the internal noise present in the system", and again that "We are tempted to regard this constant as arising from the mean internal noise in the system". The meaning of this statement is not very clear, but he seems to be arguing that, since r is proportional to $\log(I+k)$, then setting $I=0$, k is the equivalent, in light units, of the mean resting discharge. This interpretation of k is, however, incorrect, since equation (2) can be derived from any relationship of the form:

$$r = a + b\log(I+k) \qquad (3)$$

where a and b are any constants; for the constant a drops out when the difference between the two impulse-rates is taken. Gregory's interpretation of k rests on the implicit assumption that $a=0$, which there is no reason to suppose true.

A further criticism of Gregory's model is that he does not consider the variance of r. In any treatment of the threshold as a signal to noise discrimination, the variance of the response is obviously far more important than the mean. It might be possible to